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## Art. CCXXIII.—*Further Remarks on Botanical Classification.*

By Philip B. Ayres, Esq., M.D.

Thame, June 19, 1844.

Sir,

I am fearful that both your readers and yourself will desire that our discussion on botanical classification, and the relative merits of the systems now in use, should come to an early termination; and in this desire I, for one, heartily concur: not, however, that I am afraid of my opponents, or ashamed of the line of argument I have employed; but that I think the pages of your journal might perhaps be more profitably filled. Nevertheless, should Mr. Edmonston or Mr. Forster prolong the controversy, I may promise them to be found at my post.

The last letter from Mr. Edmonston (Phytol. 977) was unfortunately written before he had seen my communication in reply to Mr. Forster (Id. 960); and having been so, the main argument of my last letter on the nature of species was left untouched, or at the most but slightly glanced at as a transcendental or ultra-metaphysical notion or set of notions. But whether it be transcendental and ultra-metaphysical or not, is a matter of little importance, since to answer the purpose of Mr. Edmonston, it must be *disproved.* Much therefore of Mr. Edmonston's renewed disquisition on the nature of species, must fall to the ground, since he has taken no notice of the *species* of minerals, to which the term is fully as applicable as to plants and animals. Even Linnæus himself applied the term to minerals in his 'Systema Naturæ,' and I suppose Mr. Edmonston will not call his authority in question!

Now if *species* be applicable to minerals, the power of generating their like is, as I have before shown, merely one of the characters on which the induction is founded; and the attempt to reduce my argument to absurdity, by bringing forward hybrids between genera, orders and classes, becomes completely nugatory, for the absurdity of supposing hybrids between *species* of minerals, is most palpable!!!

But further, if Mr. Edmonston will give the matter a moment's consideration, he must see that the production of hybrids between individuals of different genera, orders and classes, is rendered impossible by the difference of the structure of such plants preventing fecundation; only those plants and animals that most nearly approach each other in structure, being capable of engendering together. But it is stated on high authority, that hybrids have been procured between individuals of different genera. Thus Treviranus describes a hybrid between Campanula divergens and Phyteuma betonicæfolia; Gaertner, one from Convolvulus sepium fecundated by Ipomœa purpurea, from Datura lævis and Metel fecundated by the henbanes and tobacco, from Glaucium luteum fecundated by the poppies, &c.; Wiegmann also obtained hybrids from Vicia faba fecundated by Ervum lens, and from Pisum arvense by Vicia sativa; lastly, Knight fecundated the almond with the pollen of the peach.*

Mr. Edmonston has admitted, in his former communication, that genera, orders and classes are pure abstractions, for he has said that Nature only creates species; but we have here two of these abstractions (in a certain sense) copulating and bringing forth hybrids. What has now become of his argument derived from hybridity?

But Mr. Edmonston further says; "It is obvious to every unprejudiced mind, that Nature has created individuals having certain common peculiarities which no other induction from individuals possesses, and to these we give the name of species." I am afraid that this is as unintelligible to me, as my statement of the two senses in which groups are natural appears to be to Mr. Edmonston; but coupling it with the following sentence from Mr. E.'s letter, we shall find that even he allows, after all, that "*species*" is an induction or abstraction, for the terms are synonymous. "The word *species* implies a congeries of individuals having certain common peculiarities and distinctions; and when we say that Nature creates *species*, we mean that she creates individuals having the properties which we attribute to the abstraction — species." This settles the question as regards *species* between Mr. Edmonston and myself; and unless Mr. Forster has something more to bring forward on the subject, the matter must be considered as finally adjusted, for Mr. Edmonston has fully, but probably unintentionally, admitted all that I could desire. I certainly did not expect to proselytize him so quickly, and I congratulate him on arriving at the only sense in which the term is applicable to the three kingdoms of Nature.

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* DeCandolle, Physiologie, tome ii. p. 703.

Mr. Edmonston has surely mistaken my meaning when he puts in my mouth the statement that "all our groupings have actually an existence in the scheme of Nature." I have merely affirmed that the capability of being thrown into such groups, is a powerful argument for the existence of a plan or scheme in the Divine Mind anterior to the creation of natural objects, and that our groups are nothing more than an attempt to approximate to the Divine plan or order. "Order is Heaven's first law," says Pope, in the double character of philosopher and poet.

The groups in all systems are metaphysical; they are creations of the mind of man, but they are natural in proportion as they place in immediate proximity those objects that most closely resemble each other, or, in other words, have the strongest affinity. But from what I have said above, it is impossible that these groups should have a positive existence in external nature; we can have no personification of any one group; classes, orders, genera, species, are entirely mental. If indeed they are mental, if no personification of them can be shown (and such cannot), then are they entirely metaphysical, and being metaphysical, why taunt me with being "ultra-metaphysical," when the subject-matter of our discourse comes within the domain of that science, and of that alone?

From what has been said above, I would undoubtedly answer Mr. Edmonston, that inasmuch as the groups of both systems are creations of man's mind, they are both artificial, but that, as throwing together those plants that bear the greatest resemblance to each other, the natural system approximates most nearly to the plan which I have supposed to exist in the Divine Mind.

Mr. Edmonston's analysis of Ranunculaceæ does much more than he intended, for it totally destroys the characters of the order. He has rejected all the characters except those derived from the seed, and I complete the work of destruction, by stating that there are numerous orders that possess solid albumen and seeds without arillus, in common with Ranunculaceæ. I am afraid that even the veriest sticklers for the old system will not go so far as Mr. Edmonston, and pronounce Ranunculaceæ a nonentity! But I object to Mr. E.'s plan of examining the characters of natural orders, *in toto*. I maintain that the whole of the characters must be retained, and that the *exceptions* must not be made the rule. There are a very few genera in which exceptions to those characters occur, and these, approximating to other orders in their characters, form those oscillatory groups that

Mr. Edmonston professes himself unable to discover. Now as to these oscillatory groups, it is a curious circumstance that Mr. Edmonston has kindly furnished me with an excellent illustration of an oscillatory group, in the succeeding page to that in which he declares he has never been able to find one! The genus Detarium has, it appears, the icosandrous stamens and drupaceous fruit of Amygdalaceæ, with the compound leaves of Leguminaceæ, and is hence intermemediate between the two orders, partaking the characters of each, and intimately connecting them. Both these orders are extremely natural, so much so, that a person completely ignorant of the principles of Botany would be able to group the species composing them together, as bearing the greatest possible resemblance to each other. Now the genus Detarium forms the oscillatory group connecting Amygdalaceæ with Leguminaceæ; belonging strictly to neither, but common to both: and hence this genus may be placed by one botanist under Amygdalaceæ, by another under Leguminaceæ, according as certain characters are valued by each. But because Detarium resembles Amygdalaceæ in every particular save its compound leaves, compound leaves form the only distinctive character between Amygdalaceæ and Leguminaceæ. This, I submit, is an example of false logic, of the drawing forth a general rule from an exception; the making the exception the rule!!!

Every one is capable of classing the common leguminous plants of our gardens and fields into one group; of seeing that the pea and bean and vetch are closely related to each other. Now the fact that the veriest ignoramus is capable of such classification shows that such relationship exists. Now on turning to Withering's Arrangement, I find all the leguminous plants very snugly ensconced under the sheltering wings of Diadelphia Decandria; but turning to the descriptions of the genera, I find that Genista, Ulex, Ononis, Anthyllis &c. are really monadelphous, while Pisum, Orobus, Lathyrus and others are as truly diadelphous. Again, any moderately intelligent person would select the grasses from any confused heap of specimens, because their resemblance is most striking; but on the Linnæan system this most natural order must be broken up. Anthoxanthum is in the same work placed in Diandria, while the remainder form part of Triandria. But in truth the grasses form part of the following Linnæan classes; Monandria, Diandria, Triandria, Tetrandria, Hexandria, Monœcia and Polygamia. This, then, is the exactitude and certainty of the Linnæan system!!! Has it really so great facilities for the detection of species? Suppose a person, who is simply acquainted with the characters of the Linnæan classes and orders, were to find a monœcious or polygamous grass, would he find it ranged under Monœcia or Polygamia? Two examples have thus been given of natural orders founded by Linnæus himself, obvious to every sagacious mind, which under this arbitrary artificial scheme are distributed through some half-dozen classes!! Let no more be said of the certainty and infallibility of the artificial scheme!

Speaking of the natural system, Mr. Edmonston gives us the two following passages in his last letter, to a comparison of which I wish to draw the attention of your readers. "What a combination of disjointed things — nay, not only disjointed, but a system affected with *fragilitas ossium*, where every bone is broken, and nothing to be seen but here a fragment and there a fragment, a little of everything and nothing complete! Can such a dismembered system, such a collection of debris, be of any real practical use? I should say, No!" But now turn over a leaf, and the following passage, so admirably in unison with the above, meets your view. "Let it be borne in mind that by these remarks I do not mean to say that the natural system is or ought to be thrown aside; let it be studied in its proper place, and it will be productive of much and lasting benefit. It can scarcely be otherwise, since it bears the impress of the great minds of a Jussieu, a Brown, a DeCandolle and a Lindley, [and I would add, which Mr. Edmonston forgets, a Linnæus]; but let it not be foisted forward where it can only disgust and perplex." What! — a combination of disjointed things, a system affected with *fragilitas ossium*, a fragmentary mass, a little of everything and nothing complete—of much and lasting benefit, and bearing the impress of so many and great names!!! Surely there must be some mistake here; such fragmentary systems are not usually the productions of great minds!! Did Linnæus project a fragmentary system, after and in preference to his own primary and more perfect system? Mr. Edmonston has forgotten himself.

A few words on a point I have left unnoticed at the commencement of Mr. Edmonston's letter, and I have done. I have called the Linnæan system a mere index (but a valuable one) to plants, and this seems to have given great offence to the followers of that school. In examining this question, we must bear in mind that the species and genera are common to all classifications; these are agreed upon by all; the difference between the rival systems exists in the higher groups alone — the classes and orders. Now if we confine ourselves to these higher groups, what do we learn of a plant by the Linnæan system? Simply the number and position of the stamens and pistils

—no reference being necessarily made to other parts of the plant. Now this seems to me very like the A, B, C of an index. But in the natural system, the whole plant must be examined before a decision can be pronounced as to the order to which the plant belongs, and thus passing in review the whole structure of the plant, much more knowledge of it must be gained than by merely examining the stamens and pistils. It is true, that in descending to the genera and species, the whole plant will be examined, but this has no reference to the particular system.

In what I have hitherto said, I have not depreciated the Linnæan system below its proper level. I have admitted its great utility in furthering our primary studies of plants; but I deny that it is an arrangement that will satisfy a philosophical mind. If it could have done so, how was it that Linnæus himself propounded a "natural system"?

I must admit that there is considerable difficulty in comprehending the natural system without assistance; but those who have heard Professor Lindley's lucid demonstrations of that system, will acknowledge that it is of all systems the most philosophical.

I fear that in this and the foregoing communications, I have used many repetitions, but I have endeavoured to avoid them as much as was consistent with the full elucidation of the subject.

I am, Sir,
Your obedt. Servant,
PH. B. AYRES, M.D.

To the Editor of 'The Phytologist.'

### ART. CCXXIV.—*Explanations of the 'London Catalogue of British Plants.'* By G. E. DENNES, Esq., F.L.S., Secretary of the Botanical Society of London.

THE editorial note appended to Mr. Sidebotham's letter (Phytol. 478), renders it expedient to offer some explanations about the 'London Catalogue of British Plants.' The comments of Mr. Sidebotham are mostly founded on misapprehension of facts. For instance, Mr. S. asserts that Impatiens fulva is held to be an introduced species, "because not found in one of the twenty" local Floras consulted. The fact is not so. The number of local Floras in which the several species occur, is given only in indication of their comparative frequency and diffusion; and without reference to the distinction made between native and naturalised species. Impatiens fulva was originally introduced to this country from America, as any botanist would have informed Mr. Sidebotham.

All will concur with the editorial wishes for uniformity of nomenclature, so far as it can now be obtained. The names in works already printed cannot be changed; and just so far as new-works introduce new names, so far is the departure from uniformity increased. The sparing adoption of newly invented names, is a characteristic feature of the London Catalogue; and there is only one instance, in the whole Catalogue, of a change in the name of a species, to wit, Glyceria loliacea, which shall presently be accounted for. In every other instance, (one or two errors of the press or pen excepted), the authority is given for the species, which shows the name to be one previously in use. The London Society, thus, cannot truly be accused of making extensive changes in nomenclature. All that was done, consisted in the selection of one from among the various names already applied to the same species by different authors. Individual botanists may think that they could have made a better selection of names; but this is a matter of individual opinion or self-complacency.

In selecting the names, the leading rule was that of taking the best known name, although, in several instances, this rule was allowed to yield to other considerations. Thus, it was a question whether preference should be given to Carex recurva, as the name best known to English botanists, or to Carex glauca, as the name best known to the botanical world in general; and the latter was adopted, because priority was in its favour, and not much confusion was likely to arise from its use. On the other hand, while Carex Goodenovii was correctly substituted for Carex cæspitosa, this latter name was not substituted for that of Carex stricta; because, to take a familiar name from one species, and then give the same name to a different species, seemed the most certain mode of causing error and confusion. A similar reason operated against the name of Equisetum fluviatile; although, it is allowed, that Mr. Newman was correct (according to strict rule) in making this transfer of the name from one species to another. But the rule of priority is valuable only in its tendency to prevent confusion, and where strict adherence to it would increase confusion, there seemed sufficient reason for the departure from it.

Glyceria loliacea is a troublesome sort of plant, which comes near Triticum in technical character, while it agrees better with a section of the Linnæan genus Poa, in general habit. In Steudel's Nomenclator nine different names are recorded for it, and others could be added. Hudson united it with the other Poas, under the name of Poa loliacea; and Glyceria being a section of the genus Poa, the plant in question is called G. loliacea in the London Catalogue. In the Edinburgh Catalogue, further subdivisions of the old genus Poa were adopted, and this plant was associated with other species of Poa or Glyceria (*Smith*) under the name of Sclerochloa. Thus, putting the two Catalogues in contrast, the increase of generic names was here made by the Edinburgh, not by the London Catalogue.

Without entering more into explanations relative to species singly, it may fairly be assumed, that the proper test of a publication like the London Catalogue, is to be found in its adaptation to the end for which it is published. This Catalogue is expressly stated to have "been prepared chiefly with the object of giving increased facility in the exchanges of specimens, regularly carried on between the Botanical Society of London and its individual members." Some probability that it is adapted to such object, is implied in the fact of its distinctive peculiarities having been suggested by a botanist, who has had many years' experience in the exchange of specimens, both at home and abroad, and who was the first to bring such printed lists into use for the purposes of these exchanges, many years ago. The most important of the peculiarities which distinguish the London Catalogue from all its predecessors, is the attempt to show the comparative scarcity of each species, by means of the local Floras; and it is supposed that few persons will deny some usefulness in this innovation.

Secondly; a distinction is made between species which probably can be obtained for exchange, and species which probably cannot be obtained; and he must be a botanist of small experience who has not found the inconvenience of a list which includes (without distinction) the names of many species of which it is impossible to obtain British specimens. Foreign botanists almost invariably ask for those species, and are disappointed when they receive a parcel without any of them.

Thirdly, the native and naturalized species are also distinguished; the usefulness of such a distinction being practically admitted by the authors of our descriptive Floras, who latterly have done the same. Useful though this be, every good botanist knows well that the distinction cannot always be made with confidence. Thus, Babington describes Chelidonium majus and Impatiens Noli-me-tangere as native species; yet Hooker marks the latter as a non-indigenous plant, and Henslow marks the former "possibly introduced by the agency of man." If Mr. Sidebotham can accomplish a task, only attempted by Smith, Hooker, Henslow, Babington, Watson and others, by all means let him perform the same, and make a "perfect" list of naturalized plants.

The continuous numbering of the specific names has been objected to, (Phytol. 933). For an index to herbaria, in which each genus only had a separate cover, the editorial preference for generic numbers would be judicious. In a duplicate store on a large scale, it is found practically more convenient to have a separate cover for each species, with the numbers marked outside, for convenience of ready reference and arrangement; since botanists apply for species and varieties, not for genera.

The London Catalogue, like its predecessors, has faults, both typographical and authorial; and it has not the novelty which Mr. Sidebotham would seem to give it credit for. A well compiled (but anonymous) Catalogue of British Plants, in the natural arrangement, and with synonymes, was published some six or eight years ago; which is still in print. G. E. Dennes.

## Art. CCXXV. — *Varieties.*

493. *The Nomenclature of Ferns in the 'British Flora' and the 'London Catalogue of British Plants.'* In your present number (Phytol. 497) a passage occurs, which is calculated to convey a wrong impression respecting the extent of dissimilarity between the names of ferns in the Flora and Catalogue. The Filices and Pteroides (an abbreviation of Pteridioides — Fern-like) are grouped under twenty-two generic names in the Catalogue, twenty of which are identical with those in the 'British Flora.' Two other generic names are necessarily added to the twenty, in consequence of two genera in the Flora being subdivided into four genera in the Catalogue. Among fifty-seven specific names of the same plants in the Catalogue, forty-eight correspond with those of the British Flora. Of the other nine specific names, five belong to additional species (or varieties "raised to the rank of species") adopted from Mr. Newman and 'The Phytologist;' four only being changed names — Trichomanes speciosum, Lastræa spinosa, Equisetum Telmateia and E. umbrosum. The first of these four was adopted on the authority of Madeira specimens, collected and labelled by Dr. C. Lemann, which appear connected with both forms of the Irish plant (speciosum and Andrewsii) through other specimens collected in the Azores. The remaining three names were taken up on the faith of Mr. Newman's investigations, first pub-

lished by that author, as I think, after the 'British Flora' was out. Though I do not know upon what authority the reviewer (*loc. cit.*) has "fathered" the London Catalogue upon my "pen," it would now be useless to disavow any connexion between them. — *Hewett C. Watson ; Thames Ditton, May* 28, 1844.

494. *Bentall's Drying-paper*. I have tried the new paper manufactured by Messrs. Bentall of Halstead, for drying plants, by placing specimens of several kinds into it, and not again examining them until they became dry, and find that it is well deserving of commendation. The colours of the flowers and leaves are well preserved. I placed eight thicknesses of paper between each layer of specimens. This paper is fully as good as, if not better than, the "*chalk-paper*," employed here, which is so much esteemed as to have been sent to distant parts of the kingdom, and even to S. America and Australia. — *Charles C. Babington ; St. John's College, Cambridge, June* 1, 1844.

495. *Note on Primula elatior*. I must request permission to enter a strong protest against Mr. Newman's endeavour (Phytol. 996) to give a new name to this plant; not from any objection to paying the honour due to Jacquin for discriminating a true species amongst these variable plants; but in order to prevent the addition of an unnecessary synonym, and also because Jacquin is undoubtedly the first botanist in modern times (that is since the time of Linnæus) who distinguished the plant specifically, and had therefore a right to give it a name. The P. veris β. *elatior* (Linn.) may have been intended to include Jacquin's plant, as well as the umbellate plants which connect the cowslip and primrose; but as Linnæus only employed the name to designate a variety, there was nothing to prevent Jacquin from giving it to a species. The question is not affected by Jacquin's belief or otherwise in the identity of his species and the Linnæan variety, for that is only an error in determining a synonym. The confusion of which Mr. Newman justly complains, is not caused by the name of *elatior* being used specifically; but by many British botanists applying it to the "spurious oxlips" so often found in this country, and fancying that they are the *species* thus named. It therefore becomes necessary to call it "Jacquin's oxlip," or that of "Bardfield," until English botanists learn to distinguish the true plant from the spurious one. I trust to hear no more of *P. Jacquinii*, Newman. It may be as well to add that Professor Henslow finds the P. elatior (*Jacq.*) in his parish of Hitcham, in Suffolk, from whence I possess specimens, by his kindness.—*Id.*

496. *Note on Anthyllis vulneraria.* Mr. Greenwood's note (Phytol. 1000) has led me to examine the flowers of this plant, and I find that on the Cambridgeshire examples they are monadelphous. One filament is free at the base and summit, but quite joined to the other nine throughout most of its length. I hope Mr. Greenwood will carefully re-examine his plant, and if it is really diadelphous, that he will allow me to examine a specimen of it.—*Id.*

497. *Note on Botrydium granulosum,* Grev. Most of the shallower ponds in this neighbourhood have become dry from the long want of rain to replenish them, and this circumstance has brought to view the above curious little algoid plant, which I have often looked out for, but could never before meet with. Walking with a friend from Warwickshire along the Henwick road near Worcester, the bed of a little pond opposite Henwick farm attracted our notice, as it appeared almost as if covered with hoar frost, though on the morning of the 3rd of June, and as we trod on it, our ears were surprized with a crackling as if of icy particles. On stooping down, however, I found that the whole bed of the pond was occupied by an aggregated growth of Botrydium granulosum, *Grev.*, the rupture of whose globules caused the noise we heard, and the frosty appearance presented to view I found was occasioned by innumerable *farinose granules* that densely covered the exterior of the green spheroidal vesicles forming the plant. These *exterior* granules seem not to have been noticed by observers, though very conspicuous, unless Sir W. J. Hooker alludes to them in his account of the Botrydium, where he says that "the membranous coat has *internally* a number of small granules," (Brit. Flor. ii. 321). When the vesicles collapse and become cup-shaped, they then indeed appear superficially internal, but they cover all sides of the globule before the fluid within it is spent. The size of the green spherical vesicles is subject to great diversity, as in all globular structures, being very minute where they are crowded in their growth by pressing upon each other, averaging generally the size of a mustard-seed; but when standing singly, nearly double that size, or as large as a currant. They have rather a tough skin, for it requires some degree of pressure to burst them by the hand, though of course breaking, with a crackling noise, beneath the tread. In general, the vesicles collapse without bursting, though when forced to do so, or when pricked with a needle, the included fluid is diffused externally, the skin sinks down, and the plant presents the appearance of a Peziza, or where there are many together, like the cells of a wasp's nest upon a small scale. Having at the time only a pamphlet to put my specimens in,

they were rather pressed before I returned home, and by the next morning, all the globules had collapsed, and their contents having dried up, the residuum appeared in the form of a green powder diffused upon the paper — doubtless the fructifying sporules of the Botrydium. This reproductive economy seems not to agree very well with the filamentose Vaucheriæ with which the present plant is associated, but rather suggests a closer relation to the Fungi, or at least the Nostochineæ. Its appearance probably depends on meteoric circumstances, in consequence of which it is seldom to be met with, and its existence even under favourable coincidences is but of short duration. The exposed bottoms of ditches in the present dry summer may possibly reveal the Botrydium in various localities to the Cryptogamic botanist; and having been thus enabled to note it, and as, according to Hooker, "few localities are published," I have thought it might be interesting to bring it forward again under the eye of the observer.—*Edwin Lees; Powick, Worcestershire, June* 8, 1844.

498. *Note on the habitat of Œnanthe pimpinelloides.* In the report of the proceedings of the Edinburgh Botanical Society (Phytol. 1005), I notice a statement by Mr. Ball, enumerating three British species of Œnanthe, and remarking, that Œ. pimpinelloides is *very rare*, the author having seen only one specimen, gathered by me at Forthampton, Gloucestershire. I should rather say the plant was of *uncertain occurrence* than very rare; as for three years that I resided at Forthampton, it was so plentiful in the two orchards adjoining Forthampton cottage, as to form no inconsiderable portion of the hay of those pastures, which are on rising ground, ascending towards some of the highest land in the parish, and the soil a dry, hard, red marl. I have not had an opportunity of inspecting the spot this year. Last season, the plant was very abundant in several meadows at Powick, where I am now unable to find a single specimen. These meadows were dry and hilly. I know nothing about Œ. Lachenalii, which Mr. Babington describes, though omitting Œ. pimpinelloides; but there is some discrepancy which requires correction, as to the habitat of the latter. This is stated by Sir W. J. Hooker, Smith, and I believe most if not all authorities, to grow in "salt marshes," while Œ. peucedanifolia is said to be found in "fresh water." This is certainly a mistake. I have never observed Œ. pimpinelloides except in inland places, on hilly, and often the *very driest ground* of the neighbourhood; while as constantly do I find Œ. peucedanifolia in *marshy* places, whether of fresh or salt water. Last year, when on the coasts of Devon and South Wales, I was very attentive to this cir-

cumstance; but the species growing in the littoral marshes there invariably proved to be Œ. peucedanifolia. The latter I also found, this spring, growing plentifully in a wet part of the island pasture called the Severn Ham, at Tewkesbury, Gloucestershire. The spherical knobs on the roots of Œ. pimpinelloides, extending some distance from the plant, are very characteristic when contrasted with the elliptical sessile knobs of Œ. peucedanifolia, and I have found them pleasantly edible. They are not represented in the 'English Botany' figure. It is indispensible to examine the roots, for the radical leaves of Œ. pimpinelloides soon wither, and then the plant, without very close investigation, can scarcely be distinguished from Œ. peucedanifolia.—*Id.*

499. *Note on Carex paradoxa?* While consulting Hooker's 'Companion to the Botanical Magazine' on another subject, a few days ago, I met with the following passage in Mr. Woods' interesting account of a 'Botanical Excursion in the North of England,' in 1835. As it relates to plants which have recently caused a good deal of discussion, I think it entitled to a place in 'The Phytologist.' Mr. Woods says: — "Rosa Doniana grows at the top of a woody bank a little above Croft, on the Yorkshire side of the river [Tees]; and near Halnaby, on the same side, there is a small strip of boggy ground, mostly covered with brush-wood, on the left hand of the road from Croft, which affords Ranunculus Lingua, and a Carex, which is perhaps a small variety of C. paniculata, but not forming dense tufts, and therefore in some degree approaching to C. teretiuscula. The beak also is not abrupt, as described in C. paniculata, but tapers gradually from the fruit. Hooker (Brit. Fl. ed. 3. p. 395) mentions a continental species, C. paradoxa, which is intermediate between these two. That species, however, is described as forming very large and dense tufts (see Gaudin, Fl. Helv. 6. 43), and therefore can have nothing to do with this plant. Some difficulty has arisen from the figure of C. teretiuscula in 'English Botany,' where the scales are altogether brown, whereas, according to Gaudin, *l. c.* the scales of C. teretiuscula in a young state have uniformly a whitish border. In my plant they have a pretty wide scariose margin." Which of the two plants can this be — C. paradoxa, which is now known to grow in another part of Yorkshire, or the variety of C. teretiuscula? Will Mr. Woods kindly examine his specimens, and favour the readers of 'The Phytologist' with the result. Or the plant may be still growing in the locality where Mr. Woods met with it, and may perhaps be found by

other botanists. — *Geo. Luxford; 2, Ebenezer Row, Kennington Lane, June* 22, 1844.

500. *Flora of the Shetland Islands.* Our correspondent Mr. T. Edmonston, jun. has forwarded to us a prospectus of his forthcoming 'Flora of the Shetland Islands,' which is to be published by subscription. Mr. Edmonston has been so long and so honourably known as a zealous and successful investigator of the Natural History of these islands, and more especially of their Botany, that we deem it unnecessary to do more at present than to announce his intention of publishing the result of his researches in an accessible form.—*Ed.*

## ART. CCXXVI. — *Notice of 'The London Journal of Botany.'* By SIR W. J. HOOKER, K.H., LL.D., F.R.A. & L.S., &c. No. 30, June, 1844. London: Baillière, Regent St.

IN a notice of the 'London Catalogue of British Plants,' in the June number of the 'London Journal of Botany,' we find some observations on the review of Mr. Newman's 'History of British Ferns' which lately appeared in our pages (Phytol. 945). These observations appear to require from us something in the shape of explanation; we shall therefore, in the first place, lay before our readers the whole of the observations referred to, and then, in all due courtesy, proceed to offer a few remarks upon such parts as seem to be not entirely consistent with facts.*

Speaking of the 'Edinburgh Catalogue of British Plants,' the edi- of the Journal proceeds: —

> "Nevertheless, as being the fullest 'Catalogue of British Plants' extant at the time of the publication of the 5th edition of the 'British Flora,' the author of that work deemed it entitled to quotation among the synonymes, 'as one in which especial pains appear to have been taken to form a *complete* list of the native flowering plants and ferns of Great Britain.' This little compliment, paid to to the labours of those who compiled the Catalogue, has been strangely made the subject of censure in a recent number of the 'Phytologist,' (a work not always distinguished by courtesy of style); where, after speaking of the 'Nomenclature' of Mr. Edward Newman, as employed in the first edition of his 'History of British Ferns,' and promulgated in 1839-40,—the reviewer assures us, 'there was a general denunciation of changes so radical and so complete;' but, after the appearance of Mr. John Smith's paper on the same subject, 'Botanists, who one month proclaimed the absurdity of Mr. Newman's inno-

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* The editor thinks it right to state that he is not the author of the review commented upon in the 'London Journal of Botany.' It was written and sent for insertion by a correspondent, who has paid great attention to the British Ferns.

vations, were seen the following month bending the supple knee to the same innovations;' and 'Dr. Balfour and Mr. Babington, by adopting the alterations, were the means of disseminating them from John o'Groats to the Land's End.' It seems the author of the *British Flora* did not bend the supple knee to the innovations, and the reviewer proceeds: 'But in the midst of its successful career, the new nomenclature met a most decided check in the publication of the fifth edition of Sir W. J. Hooker's *British Flora*, wherein we were astonished to find the changes introduced by Mr. Newman, not only *fathered* upon the authors of the '*Edinburgh Catalogue*,' but the new names given as synonymes, and *the old nomenclature restored in all its glory*."—p. 290.

To the above passage is appended the following long foot-note.

"It is far from being our general intention to notice remarks made in *reviews* of Books: but the Editor of this Journal, as the Author of the '*British Flora*,' must in justice to himself declare that he is not aware that he has in any way acted unfairly by Mr. Newman. He presumes by the expression of 'fathering the changes introduced by Mr. Newman upon the authors of the Edinburgh Catalogue,' it is meant to imply that he has given to those gentlemen a credit for names ('a nomenclature') which is due to Mr. Newman alone. But surely no one will consider that to be the case, who has seen the little explanation in the preface to the *British Flora*, (ed. 5, p. viii.). It was never meant to imply that the Editors of the Catalogue were the *authors* of those names: and really upon looking at the places among the Ferns where the '*Edinb. Cat.*' is quoted, the difference of names is so trifling that it is marvellous how such a charge could, in fairness, any way be made. One would suppose that by 'the old nomenclature being restored in all its glory,' that the author had gone back to the days of Dillenius and Ray; but, so far at least as the Edinburgh Catalogue is concerned, the difference of names, 'fathered' upon the Edinb. Cat., which Mr. Newman's reviewer claims for him, and in the *British Flora*, amounts to these. In the latter work, the genus *Aspidium* of Swartz is divided into two sections; 1st. those species with orbicular involucres, fixed by the centre (*Aspidium*, Br.), and 2ndly. those with reniform involucres, fixed by the sinus, (*Nephrodium*, Rich, Br.) The first are called *Polystichum* in the Edinburgh Catalogue, while the latter are called *Lastræa;* and in the genus *Asplenium*, as defined in *British Flora*, 2 species (*A. Filix fœmina*, and *A. fontanum*), are, in the Edinb. Cat., called *Athyrium*. *Cryptogramma* of Brown and Hook. is called *Allosorus* in the Ed. Cat.; *Blechnum boreale*, Sw., is called *Lomaria Spicant*, and *Trichomanes brevisetum*, Br. and Hook. is the *T. speciosum* in Ed. Cat. Now the whole of these changes (we are not discussing the merits or correctness of the names) no more originated with Mr. Newman, they are no more his original 'nomenclature,' which is declared 'to be *toto cœlo* at variance with that so long in use,' than they did with the authors of the Edinburgh Catalogue. The genus *Polystichum* was invented by Roth in the year 1800, and is absolutely identical with *Aspidium*, as it stands in the British Flora; including both *Polystichum* and *Lastræa* of the Edinburgh Catalogue. *Lastræ* originated in M. Bory de St. Vincent in 1824, and was formed to include the *Polypodia! Oreopteris*, *Thelypteris* and *unitum*. Presl in 1836 altered the character, to make it comprise certain Aspidiaceous plants, banished *all* Bory's species, and was the author of the names of the *Lastræa* as they stand in the Edinb. Cat. *Athyrium* also is a genus of Roth (1800), adopted (in part by Presl), and the species above mentioned are of the same antiquity. We are well aware that *Cryptogramma crispa*, Br. is the *Allosorus* of Bernhardi, (1806); but the *Cheilanthes odora*, Sw. is the plant which Bernhardi seems to have had in view in constituting that genus; and Presl has not improved the genus by the heterogeneous species he has mixed up with it, and which have little in common with the plant in question. So long ago as 1810, Mr. Brown expressed his opinion that *Blechnum boreale* might perhaps be referred to his *Stegania* (*Lomaria*, Willd.); and in 1811, Desvaux named it *Lomaria Spicant*. Our view of the fructification differs from that of these authors, and we have not preserved the name of Swartz without stating reasons for it, and giving a figure, which in our mind, at least, confirms those views, (see Brit. Fl. ed. 5, TAB. X. f. 10.) With regard to Mr. Brown's name of *Trichomanes brevisetum*, we may here extract what is but just printed respecting it in the '*Species Filicum*,' p. 126, where the author expresses his regret that he was under the necessity of occupying so much space in his attempt to unravel the difficulties which have always attended the synonymy of this plant, and respecting which Sir Jas. E. Smith (whose writings on the Ferns the reviewer, in the 'Phytologist,' treats with marked contempt), said, nearly thirty years ago, that 'few plants of almost any country have caused more enquiry, or more diversity of opinion, than this Fern.' Unquestionably the *T. speciosum* of Willd. is the same species as our *T. brevisetum*, though a native of Teneriffe; and, as such, the name has the right of priority over that of Mr. Brown; but after a most careful investigation of other specimens of *Trichomanes*, and especially the *T. radicans* of Swartz, from Jamaica, we must declare ourselves at issue with the reviewer in question, who, notwithstanding that 'Mr. J. Smith had labelled a var. of the Irish *T. speciosum*, lately discovered by Mr. Andrews, as *T. radicans*, Sw.;' nevertheless 'thinks Mr. Newman has exercised a sound discretion in keeping the name of *T. radicans* quite out of view.' The author of the '*Species Filicum*' has come to a different conclusion, and having satisfied himself of their identity, ventures to retain the name of *radicans*. It might be supposed that the reviewer was of the same mind when he says, (Phytol. p. 956), 'it was held to be impossible that a *tropical* plant should exist in Ireland.' He surely does not take Teneriffe and Madeira, the recorded habitats of *T. speciosum*, to be within the tropics. Let it be observed that Mr. Newman gives no authority for the genera *Polystichum* and *Lastræa* in his Synoptical Table of British Ferns, p. 6, but he informs us (p. 8) that these 'have not been employed by any previous writer on the British Ferns.'

"One word on another remarkable passage of the reviewer, who pronounces Mr. J. Smith's Arrangement of the genera of Ferns as 'perhaps the most profound and useful treatise ever presented to the Linnæan Society.' Knowing, as the Editor of this Journal does, the character of Mr. J. Smith, and his love of truth, he hesitates not to say that such an overstrained compliment will be far from gratifying to him. Of the merits of his Memoir, the Editor entertains a very high opinion, and of his judgment in discriminating types of genera or subgenera, and it has been held a privilege to afford publicity to that very paper in the pages of this work; and further, to give figures of Mr. Smith's new genera (see '*Genera Filicum*,' passim): but great as is his merit in the 'Arrangement,' now mentioned, it rises much higher in our esteem on account of the *candour* with which he speaks of his predecessor in the same line; and it detracts nothing from Mr. Smith's merit that Presl was his predecessor in these innovations, for the two writers worked wholly independently of each other. "I had nearly," says Mr. J. Smith, "completed my arrangement, when I received a copy of Presl's 'Tentamen Pteridigraphiæ,' a work published at Prague in 1836, but not seen by me till 1838. That author's opinions so nearly coincided with mine, that it might seem as if a communication of ideas had passed between us; but, after allowing him due credit for his labours, I must still continue to differ from him in a number of important points; yet in order to avoid adding synonymous generic names, I have revised my original ones, and in all cases, where Presl's character of his genera are conformable to my view, I have adopted his names.' "

In the following remarks we shall confine ourselves as much as possible to the plain *facts* of the case, without in general attempting any explanation of the reviewer's *opinions*. We will also endeavour to follow the order of the editor's strictures.

First, then, with regard to the "little compliment" said to be made the subject of the reviewer's censure. We do not clearly perceive in what way the simple statement of a fact can be construed into censure. That the payment of this "*little* compliment" has been in some degree censured in preceding pages of 'The Phytologist,' we are willing to admit; but in the passage alluded to we must confess ourselves unable to discover the censure complained of. When the 5th edition of the 'British Flora,' was announced, the botanists of this country naturally looked forward to its appearance with a hope that the author would gladly embrace the favourable opportunity of raising the Botany of Britain to something like a level with the state of the science on the continent. This, the author of the 'British Flora,' from his eminent attainments in the science, his almost unequalled store of materials, and his exalted station, was certainly well qualified to accomplish, and was no more than the botanists of Britain had a right to expect from him. His labour, too, would have been rendered less onerous, from the publication of the 'Edinburgh Catalogue of British Plants,' "in which especial pains had been taken," not only "to form a *complete* list of the native flowering plants and ferns of Great Britain," but also (and this surely is a point of equal importance), "as far as possible to make the nomenclature of British plants correspond with that adopted by the best continental authors." And in endeavouring to effect this object, the compilers of the Catalogue were "guided in their amendments chiefly by the works of DeCandolle, Koch, Nees von Esenbeck, Kunth and Leighton." However, when the 5th edition of the 'British Flora' appeared, great was the disappointment of our botanists to find that very little more had been done in its preparation, than to prefix a Linnæan index or key, to adopt the natural arrangement for the body of the work, in place of that of the Linnæan artificial system which had been used in the preceding editions, and to add a few explanatory plates at the end:

while, so far as nomenclature and characters were concerned, the work remained almost *in statu quo.*

In the Edinburgh Catalogue, when the name of a genus or species was changed, the compilers paid the author of the 'British Flora' the compliment of giving a reference to the name under which it appeared in the fourth edition of that work. In a mere Catalogue of names no more than this could be looked for. But, in a standard work like the 'British Flora,' if the author saw reason not to adopt the changes proposed in the Edinburgh Catalogue, we have always held the opinion, and in this we are not alone, that if he thought fit to notice the Catalogue at all, he was bound to pay the compilers a higher compliment than the somewhat equivocal one of merely deeming it "entitled to quotation among the synonyms," and that, too, simply on the ground of especial pains appearing to have been taken to render it a *complete* list of British plants. The compilers of the Catalogue, having endeavoured to place the nomenclature of British plants on a sounder basis than before, we consider that the least that could have been expected, if their changes were not adopted, was that the reasons for their rejection should have been stated; and such reasons, coming from the author of the 'British Flora,' would have been received with all the deference and respect to which, emanating from such a quarter, they were fairly entitled. The rejection of these proposed changes without such reasons being adduced, has been the only ground for censure on this subject, which may at any time have been expressed in our pages.

But, it may be asked, what has all this to do with the question at issue between the editor of the 'London Journal of Botany' and the reviewer of Newman's 'History of British Ferns.'? We answer,—Much every way!—inasmuch as among the rejected changes in nomenclature were several relating to British ferns, which, having previously been proposed and introduced by Mr. Newman, were thought worthy of adoption by the compilers of the Edinburgh Catalogue. We advisedly say, *introduced* by Mr. Newman—not *invented;* for nowhere in his 'History of British Ferns,' does he claim for himself the merit of having *invented* a single name. And in blaming the reviewer for claiming for Mr. Newman the merit of having originated or invented "a nomenclature," we cannot help thinking that the editor has been fighting with a shadow of his own raising.

We now pass on to the editor's declaration, that he is not aware of having in any way acted unfairly by Mr. Newman. Previously, perhaps not; but in the foot-note above quoted, and in one paragraph more especially, we fancy we discover such symptoms of unfair dealing, that we cannot let slip this opportunity of endeavouring to place the matter in its true light.

After enumerating the discrepancies between the Edinb. Catalogue and the 5th edition of the 'British Flora,' the editor continues: — "Now the whole of these changes, * * no more originated with Mr. Newman, they are no more his original "nomenclature," * * than they did with the authors of the Edinburgh Catalogue.* Now upon an impartial examination of the review, we cannot perceive that the reviewer anywhere claims for Mr. Newman the merit of having originated this "nomenclature," and certainly Mr. Newman nowhere claims it for himself, as we think we shall presently be able to prove, from the evidence kindly laid before us by the editor himself.

With a few exceptions, the editor might have found nearly all that he has said on the authorship of certain genera, in the 'History of British Ferns' itself. For instance, he says that "The genus Polystichum was invented by Roth in the year 1800, and is absolutely identical with Aspidium, as it stands in the British Flora! including both Polystichum and Lastræa of the Edinburgh Catalogue." So far the editor; now for Mr. Newman. At p. 36 (ed. 1) of his History, under Polystichum aculeatum, he says, "This genus was established by Roth, and has been adopted by DeCandolle and Schott;" and the following passage occurs at p. 11, 2nd edition, "Genus *Polystichum*, Roth. Involucre attached by its centre. This genus includes all the species I have placed under Lastræa; it was first restricted by Schott to the congeners of Roth's typical species—Lonchitis." If now we turn to p. 166 of the 2nd edition of the History, we find a passage bearing a remarkable family likeness to the above quotation from the editor's foot-note. "The genus Polystichum was established by Roth in his 'Flora Germanica,' in 1800, and is so far synonymous with Swartz's genus Aspidium, published in the same year, that both included a long list of the same species."

The editor proceeds: — "Lastræa originated in M. Bory de St. Vincent in 1824, and was formed to include the Polypodia! Oreopteris, Thelypteris and unitum. Presl in 1836 altered the character, to make it comprise certain Aspidiaceous plants, banished *all* Bory's species, and was the author of the names of the Lastræa as they stand in the Edinb. Cat." With reference to this passage we would ask, if Presl "banished *all* Bory's species," how it happens that Oreopteris and Thelypteris are retained, not only in the Edinburgh Catalogue, but also in Presl's own Tentamen? For in the latter, we find Thelypteris and Oreopteris holding the second and third places in his §. 2. THELYPTERIS. In his Synopsis, ed. 2, Mr. Newman gives Bory as the authority for the genus: while at p. 185 of his History, ed. 2, under L. Thelypteris, he states that "Botanists seem scarcely agreed as to the genus to which this fern should be referred. Presl places it in Bory's genus Lastræa; Schott constitutes a genus (Thelypteris) purposely to receive it, and gives to the species the name of palustris; and Sir W. J. Hooker, in the fifth edition of his 'British Flora,' makes it an Aspidium: thus we have three of the highest, as well as most recent authorities, completely at issue on the question."

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* It will be seen on a reference to the original, that the sense and bearing of the passages here quoted are in no wise affected by the omissions.

Next with regard to Athyrium. The editor's words are these: —"Athyrium also is a genus of Roth, (1800), (adopted in part by Presl), and the species above mentioned [? A. Filix femina and fontanum] are of the same antiquity." At p. 61, Mr. Newman says: —"In opposition to the views of many eminent botanists, I feel inclined to return the Lady fern to the genus Athyrium of Roth, which I think must also include the Allantodia australis of Brown. I doubt whether I can agree with Presl in placing Asplenium Halleri and fontanum in the same group."

With respect to Allosorus, the editor observes, "We are well aware that Cryptogramma crispa *Br.* is the Allosorus of Bernhardi, (1806); but the Cheilanthes odora, *Sw.* is the plant which Bernhardi seems to have had in view in constituting the genus;" Mr. Newman, "It [A. crispus] has by three eminent botanists been made the type of a new genus; viz. by Bernhardi, under the name Allosorus; by Desveux, under the name Phorobolus; and by Brown, under the name Cryptogramma The name I have adopted has the claim of priority." —p. 17.

And lastly, with regard to Lomaria: the editor says "So long ago as 1810, Mr. Brown expressed his opinion that Blechnum boreale might perhaps be referred to his Stegania (Lomaria, *Willd.*);" and in his Synopsis Mr. Newman says of Stegania, "Apparently identical with Lomaria."

We have been thus particular in quoting parallel passages, in order to show that Mr. Newman was careful to give the authorities for the generic names employed by him. To this we shall presently return.

As we do not feel disposed to lengthen this article by entering upon the discussion of the question of identity of Trichomanes speciosum, &c., we will now proceed to analyze what we cannot but look upon as an unfair attempt on the part of the editor, to fix upon Mr. Newman the intention of appropriating the authorship of genera to which he had no claim. At the end of the first paragraph of the footnote quoted above, and so far removed from any observations to which it could with propriety belong, as to have the appearance of an afterthought—a capital hit, too good to be lost,—will be found the following passage. "Let it be observed that Mr. Newman gives no authority for the genera Polystichum and Lastræa in his Synoptical Table of British Ferns, p. 6, but he informs us (p. 8) that these "have not been employed by any previous writer on the British Ferns." — Now, looking on this passage as it stands here, we ask, would not any one understand it as implying that Mr. Newman thereby intended to appropriate to himself the honour of founding these two genera? On turning however to the page indicated (in the 1st edition, though not so stated by the editor) we were grieved to find a most palpable instance of *suppressio veri* — of stating *the truth*, but not *the whole* truth —which we must consider somewhat uncandid, to say the least of it. The whole passage, fairly quoted, is this. "The name following the genus is that of a *British* [so in the original] author who has employed it. The genera, for which no authority is given, have not been employed by any previous writer on the British Ferns. The works in which the genera will be found are these," &c., followed by the names of the British authors referred to with the titles of their works. It will be seen that the omission of the first sentence by the editor, materially alters the sense; and, coupled as this is with his own remarks, so emphatically introduced by "Let it be observed"! we must suppose that the editor wished it to be understood as every person would be sure to understand it, without referring to the 'History of British Ferns' for the key. We cannot, however, comprehend why Polystichum and Lastræa alone should have been selected as examples of Mr. Newman's aptitude for genus-making, seeing that Cystopteris — the genus immediately preceding the chosen ones — is in the same category, being, like them, without any authority in the Synoptical Table.

We could easily extend these remarks to a much greater length, but by this time our readers must be heartily tired of the subject. Claiming indulgence therefore for a few observations on the last paragraph of the footnote, we will afterwards conclude.

Every one who has the pleasure of being acquainted with Mr. J. Smith, must entertain the highest regard for that gentleman, both on account of his extensive knowledge of Botany, the modesty by which his attainments are veiled, and his readiness to impart information. Mr. Smith's candour, too, in speaking of his predecessor's labours, is well known. Why, therefore, the word *candour* should have been printed in ominous italics, we cannot divine, unless, indeed it be intended to imply that there has been *a want* of candour on the part of some other party "in the same line." Now, coupling this with the preceding remarks on Mr. Newman, we must suppose that the intention of the editor has been to charge that gentleman with a want of candour in speaking of the labours of his predecessors. We fearlessly invite any unprejudiced individual to examine Mr. Newman's History of British Ferns, either the 1st or 2nd edition, and point out, if he can, a single instance in which Mr. N. has omitted to speak with candour of *his* predecessors "in the same line," or neglected to acknowledge the source from which any portion of his information has been derived. More than this, we again assert that Mr. Newman has in no case claimed the merit of having *invented* the generic names used by him in his History; and as a proof of his candour, we will conclude the subject by quoting from the 'History of British Ferns,' a parallel passage to that given by the editor from the writings of Mr. J. Smith.

"Mr. Smith, of the Royal Botanic Garden at Kew, has paid great attention to the venation of Ferns; he has prepared an essay on this subject, which I trust we shall shortly see in print. * * Mr. Smith having, in the most unreserved manner, communicated to me his own ideas on the subject of arrangement and nomenclature, I was delighted to find, that, with very few exceptions, our views were similar: in one or two instances I was obviously wrong, and in these instances I was too glad to have the opportunity of rectifying my errors by the aid of his superior knowledge of exotic genera. In a few instances we still differ, and in announcing this, I fear I shall be considered as pronouncing my own condemnation: still I venture to pursue my way, and 'by an earlier appearance in the literary horizon, give myself the chance of what the astronomers call an *Heliacal rising*, before the luminary in whose light I am to be lost shall appear.'

"In making out my list of genera I have followed no other rule than that of priority; and if, in any instance, I have departed from this rule, the departure has been entirely unintentional, and I shall be glad to be informed, in order that I may take an early opportunity of correcting my error."—p. 3, 1st edition.

Surely there is no want of candour here; whatever instances of it the editor may fancy he has perceived in other parts of Mr. Newman's writings!

In concluding these remarks, penned "more in sorrow than in anger," we must express our regret that the editor of the 'London Journal of Botany' should have taken a step which we cannot but look upon as one unworthy the position he holds among botanists. We considered it due to the editor of the Journal to give insertion in our pages to the whole of his strictures, and no less due to our readers and to ourselves, to make an attempt to clear up certain passages in the extracts, which might tend to convey erroneous impressions; and having done this, we bid farewell to an unpleasant task.

## ART. CCXXVI.—*Proceedings of Societies.*

### BOTANICAL SOCIETY OF LONDON.

*June* 7, 1844. — J. Reynolds, Esq., Treasurer, in the chair.

Specimens of Œnanthe peucedanifolia and Œ. pimpinelloides were exhibited, accompanied by notices of their distinctive characters and habitats; by Edwin Lees, Esq., F.L.S. Much uncertainty and confusion having prevailed among British authors and distributors, in regard to the distinctions between these species, the views of Mr. L., founded on good opportunities for observation, are deserving of particular attention. The following condensed abstract will explain the conclusions formed by this botanist.

First.—Œ. peucedanifolia always grows in wet places, and is found both by salt and fresh water; while Œ. pimpinelloides is found in dry ground only.

Secondly. — The characters derived from the form of the radical leaves, and the presence or absence of an involucrum, will not prove sufficient to prevent confusion; but the rounded tubercles upon the roots of Œ. pimpinelloides, will readily serve to distinguish that species from Œ. peucedanifolia, in which the tubercles are elongate and sessile.

Thirdly. — There is some difference in the fruit of the two species, though the materials in the possession of Mr. L. are not sufficient to state this with precision and certainty.

Mr. L. thus attaches the first importance to the form of the root, as a distinctive character, and the circumstance should instruct collectors to be mindful of the value of the root.

Specimens collected by Mr. L. afforded the principal reason for retaining Œ. pimpinelloides as a British species, in the London Catalogue, in preference to the adoption of Mr. Babington's change to Œ. Lachenalii; and one of the same specimens communicated to Mr. Ball, induced that excellent botanist to admit Œ. pimpinelloides as well as Œ. Lachenalii among the indigenous species, (Phytol. 1005). Three species, not two only, should therefore now be looked for, and the confusion and uncertainty may thus be removed.

Most of the specimens hitherto sent to the Society have proved quite useless through the absence of roots and fruit, but it is earnestly requested by the Council, that contributors will collect specimens, with root and fruits from as many localities as possible.

Some highly interesting examples of the Irish Saxifrages, belonging to Haworth's genus Robertsonia, were exhibited from Mr. Andrews, who had obligingly sent living plants as well as dried specimens. Two of the specimens were sent in record of the fact, lately doubted or denied by the accurate Mr. C. C. Babington, that the Pyrenean forms of S. umbrosa and S. Geum (with crenate leaves), are certainly native in Ireland; the specimen of S. Geum, indeed, being considered "even more obtusely crenate than Mr. Babington's figure (No. 8) from the Pyrenean plant." These specimens were collected "this year, from the mountains to the south of Brandon mountain, County of Kerry."

S. hirsuta is considered by Mr. Andrews to be a hybrid form between S. Geum and S. umbrosa, "as many of the varieties present characters leaning either more or less to the one species or the other." S. elegans is deemed by Mr. A. to be simply a variety of S. umbrosa. And after careful examination of the forms of S. hypnoides, he is now "satisfied that S. affinis, incurvifolia, hirta and palmata are all mere varieties, or indeed, barely deserving the name of varieties."

Read, "A Synoptical View of the British Fruticose Rubi, arranged in groups, with explanatory remarks, (Part 3)," by Edwin Lees, Esq., F.L.S. The paper was accompanied by drawings and specimens.—*G. E. D.*